12h G.M.T.	Distance from Sun.	Distance from Earth.
1729, July 31	4·065	3·121
,, Aug. 8	4·072	3·134
1730, Jan. 18	4·452	5·152
,, ,, 31	4·499	5·225

For comparison with these distances take the case of the great comet of 1861, which was observed by Mr. Otto Struve at Poulkova till 1862, May 1, when its distance from the sun was 4·46, and from the earth 4·70; or that of Mauvais's comet of 1847, followed by Bond with the great refractor at Cambridge, U.S., till 1848, April 21, and then distant from the sun 3·85, and from the earth 4·40; or again, the case of the celebrated comet of 1811, last seen by Wisniewsky at Novo-Tcherkask, 1812, August 17, when the radius-vector was 4·54, and the distance from the earth 3·50.

RÜTIMEYER ON TRACES OF MAN RECENTLY DISCOVERED IN THE INTERGLACIAL COAL-BEDS OF SWITZERLAND

THE last number of the *Archiv für Anthropologie* contains a short but important article by the eminent zoologist of Basle, Prof. Rütimeyer, on some traces of man recently discovered in the interglacial coal-bed at Wetzikon. Escher von der Linth first called attention to the fact that at several places in East Switzerland, especially on the eastern shore of Lake Zurich from Wetzikon to Utznach, and again in the neighbourhood of the Lake of Constance, there are beds of coal, which are not only covered by, but which also repose on, well-marked glacial deposits, thus clearly proving the existence of more than one period of extreme cold, as first remarked by Morlot, and since confirmed by many observers, and especially by Geikie.

These interglacial coal-beds contain numerous remains of plants and animals, among the most interesting being those of *Elephas antiquus* and *Rhinoceros merkii*. The remains of plants are indeed so numerous that Dr. Scheuermann, of Basle, has been in the habit of breaking up himself all the coal used as fuel in his house, in search of vegetable remains. In doing so he was struck on one occasion by observing a number of pointed rods lying side by side, and he placed the block of coal containing them in the hands of Prof. Rütimeyer.

Prof. Rütimeyer has now given a description and figures of these rods, from which it is clear that they have been intentionally pointed, and that they formed a portion of rough basket or wattle work. They are four in number, and are closely embedded in the coal, which they precisely resemble in colour, while the texture is that of the ordinary wood found in these coal-beds. Moreover, as is usual in such cases, the stem has been compressed, so that the section is not circular but oval. According to Prof. Schwendener, the wood is that of *Abies excelsa*. The points bear evident traces of cutting, while at one part of the rods are marks as if of a string wound round and round them. Here, then, we appear to have clear evidence of the existence of man during one of the warm intervals of the glacial epoch. J. L.

THE THEORY OF "STREAM LINES" IN RELATION TO THE RESISTANCE OF SHIPS *

III.

IN this treatment of the propositions concerning the flow of fluid through pipes, I have at length laid the necessary foundation for the treatment of the case of the flow of an infinite ocean past a submerged body. I have shown these propositions to be based on principles which are undeniable, and the conclusions from which, when in any way startling or paradoxical, you have seen confirmed by actual experiment.

I have dealt with the case of a single stream of uniform sectional area (and therefore of uniform velocity of flow) enclosed in a pipe of any path whatever; I have dealt with the case of a single stream of very gradually varying sectional area and velocity of flow; and I have dealt with the case of a combination (or faggot, as it were) of such streams, each to some extent curved and to some extent varying in sectional area, together composing the whole content of a pipe or passage having enlargements or contractions in its course; and in all these cases I have shown that, provided the streams or pipe-contents finally return to their original path and their original velocity of flow, they administer no total endways force to the pipe or channel which causes their deviations.

I am now going to deal with a similar combination of such streams, which, when taken together, similarly constitute an infinitely extended ocean, flowing steadily past a stationary submerged body; and here also I shall show that the combination of curved streams surrounding the body, which together constitute the ocean flowing past it, return finally to their original direction and velocity, and cannot administer to the body any endways force.

The argument in this case is, in reality, precisely the same as that in the case of the contractions and enlargements in pipes which I have already dealt with; for, in fact, the flow of the ocean past the stationary submerged body is only a more general case of the flow of fluid through a contracted pipe; but, though the cases are really the same, there is considerable difference in their appearance; and therefore I will proceed to point out how the arguments I have already used apply equally to this case.

Every particle of the fluid composing the ocean that passes the body must undoubtedly follow some path or other, though we may not be able to find out what path; and every particle so passing is preceded and followed by a continuous stream of particles all following the same path, whatever that may be. We may, then, in imagination, divide the ocean into streams of any size and of any cross-section we please, provided they fit into one another, so as to occupy the whole space, and provided the boundaries which separate the streams exactly follow the natural courses of the particles.

I before suggested a similar conception of the constitution of the ocean flowing past the stationary body, and there pointed out that the streams forming this system must not only be curved in order to get out of the way of the body, but might each require to have to some extent a different sectional area, and therefore a different velocity of flow at different points of their course. If we trace the streams to a sufficient distance ahead of the body, we shall there find the ocean flowing steadily on, completely undisturbed by, and as we may say ignorant of, the existence of the body which it will ultimately have to pass. There, all the streams must have the same direction, the same velocity of flow, and the same pressure. Again, if we pursue their course backwards to a sufficient distance behind the body, we shall find them all again flowing in their original direction; they will also have all resumed their original velocity; for otherwise, since the velocity of the ocean as a whole cannot have changed, we should have a number of parallel streams having different velocities and therefore different pressures side by side with one another, which is an impossible state of things.*

Although, in order to get past the body, these streams follow some courses or other, various both in direction and velocity, into which courses they settle themselves in virtue of the various reactions which they exert upon one another and upon the surface of the body, yet ultimately, and through the operation of the same causes, they settle themselves into their original direction and original velocity. Now the sole cause of the original departure of each and all of these streams from, and of their ultimate return to, their original direction and velocity, is the submerged stationary body; consequently the body must receive the sum total of the forces necessary to thus affect them. Conversely this sum total of force is the only force which the passage of the fluid is capable of administering to the body. But we know that to cause a single stream, and therefore also to cause any combination or system of streams, to follow any courses

* Address to the Mechanical Section of the British Association, Bristol, August 25, 1875; by William Froude, C.E., M.A., F.R.S. President of the Section. Revised and extended by the author. Continued from p. 93.

* In an imperfect fluid it *is* possible to have parallel streams having different velocities and the same pressures side by side with one another, because, in an imperfect fluid, change of velocity may have been communicated by friction instead of by difference of pressure.

changing at various points both in direction and velocity, requires the application of forces the sum total of which in a longitudinal direction is *zero*, as long as the end of each stream has the same direction and velocity as the beginning. Therefore the sum total of forces (in other words the only force) brought to bear upon the body by the motion of the fluid in the direction of its flow, is *zero.**

I have now shown how it is that an infinite ocean of perfect fluid flowing past a stationary body cannot administer to it any endways force, whatever be the nature of the consequent deviations of the streams of fluid. The question, what will be in any given case the precise configuration of those deviations, is irrelevant to the proof I have given of this proposition. Nevertheless it is interesting to know something, at least, of the general character which these deviations, or "stream-lines," assume in simple cases; therefore I have exhibited some in Figs. 26, 27, which are drawn according to the method explained by the late Prof. Rankine.

The longitudinal lines represent paths along which particles flow; they may therefore be regarded as boundaries of the streams into which we imagined the ocean to be divided.

We see that, as the streams approach the body, their first act is to broaden, and consequently to lose velocity, and therefore, as we know, to increase in quasi-hydrostatic pressure. Presently they again begin to narrow, and therefore quicken, and diminish in pressure, until they pass the middle of the body, by which time they have become narrower than in their original undisturbed condition, and consequently have a greater velocity and less pressure than the undisturbed fluid. After passing the middle they broaden again until they become broader than in their original condition, and therefore have less velocity and greater pressure than the undisturbed fluid. Finally, as they recede from the body they narrow again until they ultimately resume their original dimension, velocity, and pressure.

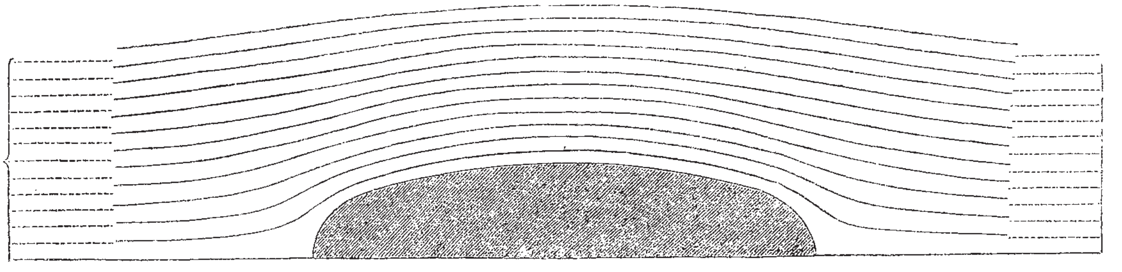

Fig. 26.

Thus, taking the pressure of the surrounding undisturbed fluid as a standard, we have an excess of pressure at both the head and stern ends of the body, and a defect of pressure along the middle.

We proved just now that, taken as a whole, the fluid pressures could exert no endways push upon the stationary body. We now see something of the way in which the separate pressures act, and that they do not, as seems at first sight natural to expect, tend all in the direction in which the fluid is flowing; on the contrary, pressure is opposed to pressure, and suction to suction, and the forces neutralise one another and come to nothing, and thus it is that an ocean of perfect fluid flowing at steady speed past a stationary submerged body does not tend to push it in the direction of the flow. This being so, a submerged body travelling at steady speed through a stationary ocean of perfect fluid will experience no resistance.

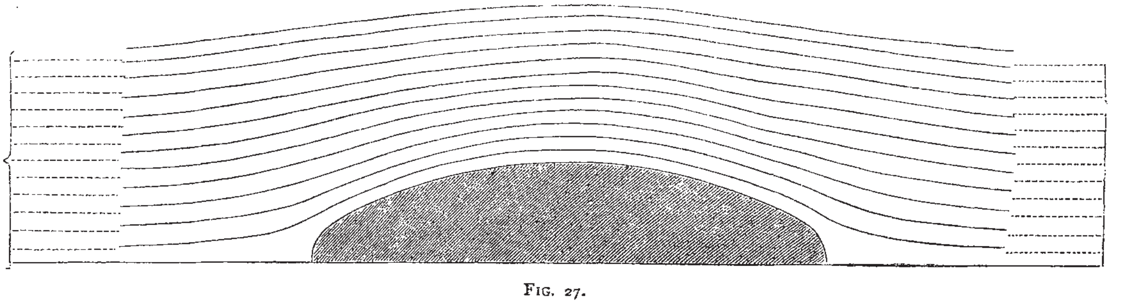

Fig. 27.

We will now consider what will be the result of substituting an ocean of water for the ocean of perfect fluid.

The difference between the behaviour of water, and that of the theoretically perfect fluid is twofold, as follows :—

First. The particles of water, unlike those of a perfect fluid, exert a drag or frictional resistance upon the surface of the body as they glide along it. This action is commonly termed surface-friction, or skin-friction; and it is so well-known a cause of resistance that I need not say anything further on this point, except this, that it constitutes almost the whole of the resistance experienced by bodies of tolerably easy shape travelling under water at any reasonable speed.

Secondly. The mutual frictional resistance experienced by the particles of water in moving past one another, combined with the almost imperceptible degree of viscosity which water possesses, somewhat hinders the necessary stream-line motions, alters their nice adjustment of pressures and velocities, and thus defeats the balance of stream-line forces and induces resistance. This action, however, is imperceptible in forms of fairly easy shape. On the other hand, angular or very blunt features entail considerable resistance from this cause, because the stream-line distortions are in such cases abrupt, and degenerate into eddies, thus causing great differences of velocity between adjacent particles of water, and great consequent friction between them.

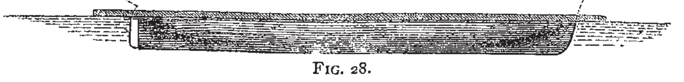

Fig. 28.

"Dead water," in the wake of a ship with a full run, is an instance of this detrimental action.

So far we have dealt with submerged bodies only; we will now take the case of a ship travelling at the surface of the water. But first, let us suppose the surface of the water to be covered with a sheet of rigid ice, and the ship cut off level with her water-line, so as to travel beneath the ice, floating, however, exactly in the same position as before (see Fig. 28). As the ship travels along, the stream-line motions will be the same as for a submerged body, of which the ship may be regarded as the lower half; and the ship will move without resistance, except that due

* See Supplementary Note C.

to the two causes I have just spoken of, namely surface-friction and mutual friction of the particles. The stream-line motions being the same in character as those we have been considering, we shall still have at each end an excess of pressure which will tend to force up the sheet of ice, and along the side we shall have defect of pressure tending to suck down the sheet of ice. If, now, we remove the ice, the water will obviously rise in level at each end, so that excess of hydrostatic head may afford the necessary reaction against the excess of pressure; and the water will sink by the sides, so that defect of hydrostatic head may afford reaction against the defect of pressure.

The hills and valleys thus formed in the water are, in a sense, waves; and, though originated in the stream-line forces of the body, yet when originated they come under the dominion of the ordinary laws of wave-motion, and, to a large extent, behave as independent waves.

The consequences which result from this necessity are most intricate; but the final upshot of all the different actions which take place is plainly this—that the ship in its passage along the surface of the water has to be continually supplying the waste of an attendant system of waves, which, from the nature of their constitution as independent waves, are continually diffusing and transmitting themselves into the surrounding water, or, where they form what is called broken water, crumbling away into froth. Now, waves represent energy, or work done; and therefore all the energy represented by the waves wasted from the system attending the ship, is so much work done by the propellers or tow-ropes which are urging the ship. So much wave-energy wasted per mile of travel is so much work done per mile; and so much work done per mile is so much resistance.

The actions involved in this cause of resistance, which is sometimes termed "Wave-Genesis," are so complicated that no extensive theoretical treatment of the subject can be usefully attempted. All that can be known about the subject must, for the present, I believe, be sought by direct experiment.

Having thus briefly described the several elements of a ship's resistance, I will proceed to draw your attention more particularly to certain resulting considerations of practical importance. Do not, however, suppose that I shall venture on dictating to shipbuilders what sort of ships they ought to build: I have so little experience of the practical requirements of shipowners, that it would be presumptuous in me to do so; and I could not venture to condemn any feature in a ship as a mistake, when, for all I know, it may be justified by some practical object of which I am ignorant. For these reasons, if I imply that some particular element of form is better than some other, it will be with the simple object of illustrating the application of principles, by following which it would be possible to design a ship of given displacement, to go at given speed, with minimum resistance, in smooth water—in fact, to make the best performance in a "measured mile" trial.

I have pointed out that the causes of resistance to the motion of a ship through the water are: first, surface-friction; secondly, mutual friction of the particles of water (and this is only practically felt when there are features sufficiently abrupt to cause eddies); and thirdly, wave-genesis. I have also shown that these are the *only* causes of resistance. I have shown that a submerged body, such as a fish, or torpedo, travelling in a perfect fluid, would experience no resistance at all; that in water it experiences practically no resistance but that due to surface-friction and the action of eddies; and that a ship at the surface experiences no resistance in addition to that due to these two causes, except that due to the waves she makes. I have done my best to make this clear: but there is an idea that there exists a kind of resistance, a something expressed by the term "direct head-resistance," which is independent of the above-mentioned causes. This idea is so largely prevalent, of such long standing, and at first sight so plausible, that I am anxious not to leave any misunderstanding on the point.

Lest, then, I should not have made my meaning sufficiently clear, I say distinctly, that the notion of head-resistance, in any ordinary sense of the word, or the notion of any opposing force due to the inertia of the water on the area of the ship's way, a force acting upon and measured by the area of midship section, is, from beginning to end, an entire delusion. No such force acts at all, or can act; as throughout the greater part of this address I have been endeavouring to explain. No doubt, if two ships are of precisely similar design, the area of midship section may be used as a measure of the resistance, because it is a measure of the size of the ship; and if the ships were similar in every respect, so also would the length of the bowsprit, or the height of the mast, be a measure of resistance, and for just the same reason. But it is an utter mistake to suppose that any part of a ship's resistance is a direct effect of the inertia of the water which has to be displaced from the area of the ship's way. Indirectly the inertia causes resistance to a ship at the surface, because the pressures due to it make waves. But to a submerged body, or to the submerged portion of a ship travelling beneath rigid ice, no resistance whatever will, be caused by the inertia of the water which is pushed aside. And this means that, if we compare two such submerged bodies, or two such submerged portions of ships travelling beneath the ice, as long as they are both of sufficiently easy shape not to cause eddies, the one which will make the least resistance is the one which has the least skin surface, though it have twice or thrice the area of midship section of the other.

The resistance of a ship, then, practically consists of three items—namely, surface-friction, eddy-resistance, and wave-resistance.

Of these the first-named is, at least in the case of large ships, much the largest item. In the *Greyhound*, a bluff ship of 1,100 tons, only 170 feet long, and having a thick stem and sternposts, thus making considerable eddy-resistance, and at ten knots visibly making large waves, the surface-friction was 58 per cent. of the whole resistance at that speed; and there can be no doubt that with the long iron ships now built, it must be a far greater proportion than that. Moreover the *Greyhound* was a coppered ship; and most of the work of our iron ships has to be done when they are rather foul, which necessarily increases the relative importance of the surface-friction item.

The second item of resistance, namely the formation of eddies, is, I believe, imperceptible in ships as finely formed as most modern iron steamships. Thick square-shaped stems and sternposts, more especially the latter, are the most fruitful source of this kind of resistance.

The third item is wave-resistance. To this alone of the three is the stream-line theory directly relevant, and here, as we have seen, it rather suggests tendencies, than supplies quantitative results, because, though it indicates the nature of the forces in which the waves originate, the laws of such wave-combinations are so very intricate, that they do not enable us to predict what waves will actually be formed under any given conditions.

There are, however, some rules, I will not call them principles, which have to some extent been confirmed by experiment. At a speed dependent on her length and form, a ship makes a very large wave-resistance. At a speed not much lower than this, the wave-resistance is considerably less, and at low speeds it is insignificant. Lengthening the entrance and run of a ship tends to decrease the wave-resistance; and it is better to have no parallel middle body, but to devote the entire length of the ship to the entrance and run, though in this case it be necessary to increase the midship section in order to get the same displacement in a given length.

With a ship thus formed, with fair water-lines from end to end, the speed at which wave-resistance is accumulating most rapidly, is the speed of an ocean-wave the length of which, from crest to crest, is about that of the ship from end to end.

I have said we may practically dismiss the item of eddy-resistance. The problem, then, to be solved in designing a ship of any given size, to go at a given speed with the least resistance, is to so form and proportion the ship that at the given speed the two main causes of resistance, namely surface-friction and wave-resistance, when added together, may be a minimum.

In order to reduce wave-resistance we should make the ship very long. On the other hand, to reduce the surface-friction we should make her comparatively short, so as to diminish the area of wetted skin. Thus, as commonly happens in such problems, we are endeavouring to reconcile conflicting methods of improvement; and to work out the problem in any given case, we require to know actual quantities. We have sufficient general data from which the skin resistance can be determined by simple calculation; but the data for determining wave-resistance must be obtained by direct experiments upon different forms to ascertain its value for each form. Such experiments should be directed to determine the wave-resistance of all varieties of water-line, cross section, and proportion of length, breadth, and depth, so as to give the comparative results of different forms as well as the absolute result for each.

An exhaustive series of such experiments could not be tried with full-sized ships; but I trust that the experiments I am now

carrying out with models, for the Admiralty, are gradually accumulating the data required on this branch of the subject.

I wish in conclusion to insist again, with the greatest urgency, on the hopeless futility of any attempt to theorise on goodness of form in ships, except under the strong and entirely new light which the doctrine of stream-lines throws on it.

It is, I repeat, a simple fact that the whole framework of thought by which the search for improved forms is commonly directed, consists of ideas which, if the doctrine of stream-lines is true, are absolutely delusive and misleading. And real improvements are not seldom attributed to the guidance of those very ideas which I am characterising as delusive, while in reality they are the fruit of painstaking, but incorrectly rationalised, experience.

I am but insisting on views which the highest mathematicians of the day have established irrefutably; and my work has been to appreciate and adapt these views when presented to me.*

No one is more alive than myself to the plausibility of the unsound views against which I am contending; but it is for the very reason that they are so plausible that it is necessary to protest against them so earnestly; and I hope that in protesting thus, I shall not be regarded as dogmatic.

In truth, it is a protest of scepticism, not of dogmatism; for I do not profess to direct anyone how to find his way straight to the form of least resistance. For the present we can but feel our way cautiously towards it by careful trials, using only the improved ideas which the stream-line theory supplies, as safeguards against attributing this or that result to irrelevant or, rather, non-existing causes.

(*To be continued.*)

THE CHANNEL TUNNEL—SUBMARINE EXPLORATIONS†

AN important Report in connection with the proposed Channel Tunnel has just been issued by the French Submarine Railway Association, giving the results of a detailed examination of the French coast, and of soundings taken in the bed of the Channel during the past autumn. The subject has on previous occasions been referred to in the pages of this Journal;‡ but before giving an account of the recent explorations it may be well briefly to refer to what has already been done.

A tunnel under the Channel has long been talked of, and many schemes have been brought forward; but the only one which has been received with general favour is that of Sir J. Hawkshaw, who proposes to carry the tunnel from the South Foreland to near Sangatte. In 1864 Mr. E. C. H. Day was employed by Sir J. Hawkshaw to make a geological survey of the neighbouring coasts with the view of obtaining some guide as to the probable outcrops beneath the Channel; the map thus produced was published with the early statements of the Company. In 1866 borings were made on both coasts to prove the succession of the strata at points near which the tunnel was to leave the shores; that on the shore at St. Margaret's Bay traversed 240 feet of upper chalk and 296 feet of lower chalk, and was stopped in the gault at a depth of 567 feet from the surface. The boring on the French coast was put down a little north of Sangatte; it passed through 70 feet of drift-sand, &c., 190 feet of chalk with flints, 284 feet of chalk without flints, and was stopped at a depth of 551 feet from the surface owing to an accident to the hole. This boring, therefore, did not reach the Upper Greensand, and the depth to this bed was estimated from information obtained in the deep boring at Calais. This accident was unfortunate, because, owing to a misreading of the accounts of the Calais boring, I believe that the thickness of the lower chalk was considerably over-estimated at Sangatte.

At a later date soundings were taken along the line of the proposed tunnel, and at varying distances to the south-west of that line; the instrument used penetrated the bottom for a few inches, and brought up specimens of the ocean floor. The larger number of these were from the superficial covering (sand, &c.), but many brought up pure chalk, and several specimens of gault were obtained near the English coast. This examination was not detailed enough to test very severely the geological map; but so far as the information went it tended to confirm the previous surveys; the only difference then observed was that the gault appeared to run rather further north towards Dover than would have been expected. But it may be doubtful whether such small borings always give trustworthy evidence on this point. The lowest beds of chalk are very clayey, and when thoroughly saturated with water are often quite dark and bluish in colour. In fact, these lowest beds, when freshly exposed in railway cuttings, have been at first mistaken for gault by good observers.

No further explorations have been made till the present year. The concession to the Company was voted by the National Assembly on the 2nd of August, and was signed by Marshal MacMahon on the 5th of the same month. Anticipating the result of the vote the Company commenced work in July. By means of a steamer, soundings were taken on the bed of the Channel. A tube was fixed at the bottom of the sounding-lead, by means of which specimens were brought up. Various appliances were used, but tubes of from 20 to 22 millimètres in diameter, and 15 to 20 centimètres long were found to give the best results. The number of soundings taken per day varied according to circumstances; it averaged 70 or 80, but sometimes reached 100.

The Commission entrusted with the explorations was presided over by M. Lavalley, and consisted of MM. Delesse, Potier, and Lapparent as geologist, and M. Larousse as hydrographer. The position of the boat was at each observation carefully determined by bearings on landmarks. Every specimen collected was marked and sent to Paris for future determination and reference. In all 1,522 soundings were made; 753 specimens of the bottom were obtained, of which 335 have been determined with certainty.

The results show that the outcrop of the gault makes a bend to the north just off the French coast. The Commission carefully tested this district by divers (the water being shallow), and they believe that this bend is due to an anticlinal fold of the strata, and not to a fault; the dip of the beds probably not exceeding 10°. From the French waters across the Channel as far as the observations went (to within about five miles of the English coast), the beds run with great regularity. Supposing the observations to be trustworthy, there cannot be a transverse fault of any magnitude along this line. But the outcrop of the gault lies further to the south than was expected; in fact, it is striking direct for Folkestone church. As before remarked, the earlier observations showed the gault near the English shore to run a little further to the north than was expected; so that here there must either be a roll of the beds or a fault with a downthrow to the south-east.* The engineers point out the importance of following up this inquiry, and doubtless it will be done as early as possible next year.

No one has expected to tunnel through twenty miles of chalk without meeting with a fault, and therefore the possibility of encountering one near the English shore need cause no uneasiness. It may give no extra trouble, or yield no more water than the rest of the work. Faults are often cut in coal workings under the sea, but they do

* I cannot pretend to frame a list of the many eminent mathematicians who originated or perfected the stream-line theory; but I must name, from amongst them, Prof. Rankine, Sir William Thomson, and Prof. Stokes, in order to express my personal indebtedness to them for information and explanations, to which chiefly (however imperfectly utilised) I owe such elementary knowledge of the subject as alone I possess.

† Chemin de Fer Sous-Marin entre la France et l'Angleterre. Rapports sur les Sondages exécutés dans le Pas de Calais en 1875. Fol., Paris, 1875.

‡ Vol. i. pp. 160, 303, 631. Prof. Hébert made a communication to the meeting of the British Association at Bristol, on the folds likely to occur beneath the Channel. (See NATURE, vol. xii. p. 407.)

* Mr. G. H. Kinahan, writing to me last year, expressed his belief in the existence of a considerable fault in the Channel, with a downthrow to the south-east.